reasons can be: I only know that they must be of a totally different order from those which constituted the substance of his published criticism.

Of course the question whether or not these physiological varieties *do* occur is quite distinct; and I most heartily agree with Mr. Wallace that this is a question of fact which ought to be decided, before it can be worth anybody's while to attack my suggestion upon any other grounds. If Mr. Wallace had seen this in the first instance, he might have saved both himself and me a good deal of trouble; but at the same time he would have deprived me of no small amount of encouragement. For I am now more than ever satisfied that the suggestion does not admit of being assailed on any grounds of general reasoning; but, on the contrary, that as a theory it is antecedently probable, and can only be refuted—if it is to be refuted—by an appeal to fact in the form of experiment. And as I cordially hope that this may be the last time that I shall have to address you upon this subject, I should like to neutralise the discouraging influence on experimental verification which may have been exercised by premature criticism in your pages. This I hope in some measure to effect by making two remarks. The first is that my own estimate of the antecedent probability of the theory is shared by some of the highest "authorities" on the Continent. The second is that, in all the lines of inquiry hitherto pursued, I find striking evidence of the actual occurrence of the physiological varieties in question. But as this evidence requires to be largely supplemented by experiment, and as every experiment requires at least three years to perform, those biologists who think with Mr. Wallace may be glad to hear that it will be a very long time before I shall have occasion again to trouble them with the theory of physiological selection.

George J. Romanes

## The Alleged Ancient Red Colour of Sirius

With reference to your paragraph last week (p. 378), in the "Astronomical Column," on "The Alleged Ancient Red Colour of Sirius," it does not seem to have been noticed that the early observations of Sirius were made at its heliacal rising. Under these circumstances the sun is a red star.

F.R.S.

## A Green Light at Sunset

At sunset to-night I observed a phenomenon which has, I believe, been seen from on board ship, but never probably from a place with such a distant sea-horizon as we have here—some seventy miles. The sky for a short distance above the point where the sun set was perfectly clear of cloud or haze, and I watched carefully the last portion of its disk disappear into the sea. As soon as the last speck of the yellow vanished, a momentary bright green flash shone out. This was quite different from the complementary green seen after looking at the setting sun; brighter and bluer in tint. I have seen it stated that the cause of this green light is the sun shining *through* the water that hides it, and would be glad to know if such is the true explanation.

R. T. Omond

Ben Nevis Observatory, February 12

## Sunset Phenomenon

On February 21, at 5.25 local time, my attention was attracted by a bright red glow reflected from the earth outside a window having an eastern aspect.

On going out of doors to the Observatory, it was evident that this crimson light proceeded from a band of cloud about 10° in width forming a great circle in the heavens, and intersecting the horizon at points, as well as I could estimate, 145° W. and 35° E. of true north, the inclination of this great circle to the horizon being about 15° or 20°.

In less than three minutes, before I could reach the Observatory, the magnificent spectacle had completely vanished, and in the place that it had occupied were merely some streaks of cirrus and cirro-stratus, the latter being nearest to the place where the sun had set, and in half an hour the entire heavens were cloudless.

Will reflection, or refraction, or both, suffice to explain the above?

Wentworth Erck

Shankhill, Co. Dublin

## Aspects of Clouds

In Mr. Ruskin's "Modern Painters" (i. Part 2) I have noticed the following passage amongst the author's remarks on the aspects of clouds:—

"I have often seen the white, thin, morning cloud edged with the seven colours of the prism. I am not aware of the cause of this phenomenon; for it takes place not when we stand with our backs to the sun, but in clouds near the sun itself; irregularly and over indefinite spaces, sometimes taking place in the body of the cloud. The colours are distinct and vivid, but have a kind of metallic lustre upon them."

And again, the author describes the "scattered mists rallying in the ravines and floating up towards you along the winding valleys till they couch in quiet masses, *iridescent* with the morning light, upon the broad breasts of the higher hills."

Dr. Johnstone Stoney recently read a paper to the Royal Dublin Society entitled "The Iridescent Phenomena in Clouds," wherein he explains the cause of a somewhat similar appearance which clouds at times present. Their outer portions are suffused with soft shades of colour like those of mother-of-pearl, a lovely green being generally conspicuous. The tints are usually distributed in irregular patches as in mother-of-pearl, but in some cases they form a regular fringe. Dr. Stoney explained that these phenomena are due to particles of ice, in the form of crystals of various sizes and shapes, and according to their position and character the sun's rays are reflected through them in various colours, thus producing the beautiful effect.

Would this be an explanation of the appearances to which Mr. Ruskin refers?

Robert James Reilly

Boyle, Ireland, February 17

## A Recently-Discovered Deposit of Celestine

It may, perhaps, be worth mentioning that a large and valuable layer of celestine has been lately found at Yate, in Gloucestershire. It lies just below the sub-soil upon a bed of red Triassic marl, which rests unconformably upon the coal-measures, just at the eastern edge of the Bristol coal-field in that district.

The deposit is, for the most part, about half a metre or more in thickness, and consists chiefly of loose nodules which, when broken, are seen to be masses of white, crystalline, nearly pure celestine. Geodes are occasionally found, one of which, about 15 cm. in diameter, lined with fine clear crystals, is now in our school museum. Beautifully transparent, though not well crystallised masses of selenite also occur in the deposit, and in these are sometimes inclosed single detached crystals of celestine. I picked out one crystal (though it seemed almost sacrilege to break up the fine specimen of selenite), which is about 7 × 4·5 × 1·5 cm., doubly terminated, fairly clear, and showing very perfect, well-developed faces. Its density is 3·95, and it shows very perfectly the characteristic light-bluish tinge of celestine.

It would, I think, be quite worth while for any mineralogist who happens to pass near Bristol to pay a visit to the place, which is only about twelve miles distant on the Midland Railway to Gloucester. The extent of the deposit is not known, but when I was there in October last, and again at Christmas, it was being worked in several fields north-east of the church, about a mile and a half from the station. Large quantities are being sent away, of course for the purposes of sugar-refining and adulterating white-lead paint.

H. G. Madan

Eton College, February 21

## "Culminating Sauropsida"

It is with satisfaction that I note in Nature of February 3 (p. 331), that Prof. W. K. Parker finds it more and more impossible "to conceive of birds as arising *direct* from the Dinosaurians, or indeed from any other order or group of reptiles." The sentence, no doubt, suggests an indirect origin of birds from reptiles; but, further on, Prof. Parker explains that if proto-vertebrate forms existed it is quite supposable that a metamorphosis may "have taken place of this and that *quasi*-larval form into archaic reptile, ancestral bird, or primitive mammal." We must therefore conclude, either that there were two kinds of protovertebrates, namely, piscine and reptilian—or ichthyopsidan and sauropsidan, as Prof. Parker would probably prefer to call them—fundamentally distinct or preceded by common ancestors,

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and in neither case themselves entitled to be called protovertebrates, or else that the protovertebrates referred to were ichthyopsida, that is to say, more simply, allied to the amphibia. I do not object to that latter supposition. I suggested it myself in 1884 (*Journal of Anatomy and Physiology*, xviii. p. 356), as perhaps Prof. Parker is aware. But if birds are developed from amphibians or pre-amphibians, and if Prof. Huxley is right, as I believe he is, in supposing that the connection of mammals with amphibians is neither through reptiles nor birds, we come to this: that amphibians or pre-amphibians have furnished the common stem whence reptiles, birds, and mammals have diverged. In that case there is an end of that group, "Sauropsida," which the birds are alleged by Prof. Parker to "culminate."

But, further, amphibians are certainly more closely allied to reptiles than to either birds or mammals. Cuvier's system may therefore be justly reverted to, and the Amphibia or Batrachia be considered as the lowest division of the Reptilia, which I do not for one moment doubt is the true classification.

University, Glasgow, February 8 JOHN CLELAND

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### The West Indian Seal (*Monachus tropicalis*)

IT will probably be of interest to the zoological portion of your readers to learn of the re-discovery—or the full discovery—of the West Indian seal (*Monachus tropicalis*). The history of this pinniped is in brief as follows.

It was first noticed by Columbus in his account of his second voyage (1494) as having been found in some numbers on the rocky isle of Alta Vela, off the southern shore of Hispaniola, where his sailors killed eight of them for food. Later—in 1675—Dampier found this seal in abundance on the Alacram reefs, about 80 miles north of Yucatan. At that time it was killed there in great numbers for its oil.

The seal then remained unnoticed for over a century and a half, having no place whatever in the writings of zoologists until 1843. Then Mr. Richard Hill published an account of it in the "Jamaica Almanac," calling it the Pedro seal, from the Pedro Keys, some 60 miles south of Kingston, Jamaica, where he had found it. A few years later Mr. P. H. Gosse obtained an imperfect skin (without skull) which he sent to the British Museum, where it was described by Dr. Gray in the Proceedings of the Zoological Society of London, 1849. Dr. Gray gave it then no name, probably by reason of its imperfect characters. Later—in 1850—(Catalogue of Mammals in the British Museum) he described this same specimen as *Phoca tropicalis*, and afterwards (Catalogue of Seals and Whales, 1866) as *Monachus tropicalis*. But so imperfect was the specimen on which the description was founded, and the animal itself was so little known, that even its generic relations were in doubt, and its reference to the genus Monachus was considered provisional. From thence on to the present, rumours of the existence of this seal have been not unfrequent, but nothing seemed trustworthy and positive, and no specimens were obtained, if we except a young skin, without bones or skull, which came from Cuba to the National Museum at Washington, in 1884, without any indication as to locality.

It has long seemed to the writer—as, doubtless, to many others—that the certain presence in our waters of so important a mammal lying *perdu* in regions which our naturalist collectors are yearly visiting, was the opprobrium of American zoologists. We made inquiries, and collected notes from many sources, which showed clearly that this seal existed at isolated points—on small islands and keys—not only in the Caribbean and among the Bahamas, but also in the Gulf of Mexico. Last summer, while on a visit to the western shore of the Gulf of Mexico, we were so fortunate as to *locate* this seal with much certainty. This was upon the Triangles (Los Triangulos), three little keys, hardly above the water-level at high tide, and lying some 100 miles north-west off the Campeachy coast, in latitude N. 20° 50′, and longitude W. 92° 10′. Following this clue, my son, Mr. Henry L. Ward, last December visited the Triangles in company and partnership with Señor F. Ferrari Perez, naturalist of the Mexican Geographical and Exploring Expedition. His hunt was highly successful, and he has during the present month returned with nearly twenty specimens—skeletons and skins of all ages, from a suckling to the fully adult male, 7 feet in length. This ample material has just been carefully studied by Prof. J. A. Allen, the well-known zoologist, and author of the "Monograph of North American Pinnipeds." Prof. Allen has given a preliminary notice of the specimens in *Science*, January 14, 1887, and promises an elaborate account, with plates, in an early issue of the Bulletin of the American Museum of Natural History, New York.

It is a fact of rather peculiar interest that this, the first large mammal ever discovered in America, should, by the strange mishaps of natural history collecting, be the very last one to become known satisfactorily to science. HENRY A. WARD

Rochester, N.Y., January 30

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### An Abnormal *Hirudo medicinalis*

WHILST dissecting the leech in the class of practical zoology, one of my students directed my attention to an apparent abnormality in the specimen which it fell to his lot to dissect. On careful examination it was found that the vesicula seminalis of the right side had moved forwards into the fifth somite, and there opened into the base of a second and fully-developed penis, which opened to the exterior on the second annulus of the fifth somite. From the vas deferens, however, there passed off to the normal penis a duct which had on it a swelling corresponding in position to the vesicula seminalis, which had been moved forwards. The various parts on the left side, as well as the female organs, were quite normal.

R. J. HARVEY GIBSON

Biological Laboratory, University College, Liverpool,
February 14

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### Instinctive Action

SOME years ago I was about to drown a terrier pup of about a month old. I held it across the palm of my open hand over a large tub of water. It lay quite still on my hand as I gently lowered it. When within 4 inches of the surface, but not yet touching the water, it deliberately began, and continued as long as I held it there, the paddling motion with its feet peculiar to dogs when swimming, and quite unlike that of walking, although I am perfectly certain this puppy had never seen or touched water before. We know almost all animals swim when first placed in water, but how could this puppy know before it touched the water that this peculiar action would be necessary? Has a similar case been observed by any of your readers?

Birmingham, February 17 D. W. C.

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## *THE RELATIONS BETWEEN GEOLOGY AND THE MINERALOGICAL SCIENCES*[1]

### I.

THE realm of Nature has been recognised from time immemorial as consisting of three kingdoms: dealing with the affairs of these three kingdoms, respectively, there have grown up side by side three departments of natural knowledge—zoology, botany, and mineralogy. But in recent years new and, I cannot help thinking, regrettable relations have sprung up between these sister sciences. Zoology and botany, having developed a method, a classification, and a nomenclature, based on common principles, have been drawn together by bonds so close and firm that many regard them as indissolubly one—the science of biology. Mineralogy, thus isolated, has been driven to seek new and unnatural alliances—with chemistry, with physics, or with the mathematical sciences. For my own part I confess that I regard this threatened "Repeal of the Union" of the natural sciences as alike a misfortune and a mistake.

It is sometimes assumed that the objects dealt with by zoology and botany are so different in their essential characters from those treated of by mineralogy, that the science of "organic" Nature must always follow a different path from that pursued by the science of "inorganic" Nature. The structures commonly known as *organic*, and the processes usually called *vital*, are asserted to be so entirely different, alike in their origin and in their essence,

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[1] Address to the Geological Society at the Anniversary Meeting on February 18, by the President, Prof. John W. Judd, F.R.S.

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